
SCIENCE AND THE MEANINGS OF TRUTH

by the same author

★

ART AND SCIENTIFIC THOUGHT
TIME, KNOWLEDGE AND THE NEBULAE

To
JESSIE

SCIENCE
AND THE MEANINGS OF TRUTH

Studies introductory to asking what is meant today by physical explanation of Nature, by mechanisms of cause and effect, and by a claim that scientific knowledge is true

by

MARTIN JOHNSON

M.A., D.Sc.

FABER AND FABER LIMITED
24 Russell Square
London

First published in Mcmxlvi
by Faber and Faber Limited
24 Russell Square London W.C.1
Printed in Great Britain by
Latimer Trend & Co Ltd Plymouth

CONTENTS

CONTENTS

PART TWO

PHILOSOPHICAL PROBLEMS OF THE MEANING OF SCIENTIFIC TRUTH:

'What are the possible connections between perceptions in ordinary experience and in scientific measurement, and the notion of "things underlying our sensations" and "things conceived in any scientific explanation of Nature"?'

Some theories of knowledge, which although inconclusively devised to account for all experience, may be capable of adaptation to the particular kind of experience which in Part One was shown to supply the communicable patterns of physics.

PART THREE

RELEVANCE OF SCIENTIFIC TRUTHFULNESS TO THE ANTITHESIS BETWEEN LOGIC AND IMAGINATION

Preface

MEANINGS OF SCIENTIFIC TRUTH

This essay arises from two convictions, firstly that recent self-criticism within physics allows or even demands a more definite meaning to the notion of Truth in science than has been hitherto possible, and secondly that any such meaning can be and must be expressed in language accessible to the general reader and not merely to the professional physicist or philosopher. But the treatment must not be 'popularized' in any sense of hiding or shirking the difficulties.

Faced with the fact that a world explored by our senses is explained in terms of electrons of a wave character and of a particle character alternately, or in terms of electromagnetic waves IN space, or in terms of curvature OF space, many suspect that scientific explanation may be true or untrue in some meaning not covered by the traditional progression from hypothesis to natural law through causes and a mechanism. Thoughtful general inquirers, with the philosopher as spearhead, ask the physicist what he means by the 'truth' of such explanations, and the more intelligent of the physicist's own students are not so readily intimidated into silence as are those general inquirers to whom mathematical language is a barrier. Now Mr. Bertrand Russell has not only been one of the most persistent philosophical observers of the physicist's progress, but he is notoriously the most readable and clearly expounding of writers, even to those who disagree with him: he attacks the most intimate of scientific problems without hiding either in a tangle of technicality or a haze of popularization. It is in the spirit of such writings, even if too inadequately to be recognized as a follower, that I am attempting here to adapt some of his clear logical analysis of the quantitative forms of reasoning, and apply them to the era of Heisenberg and Dirac in atomic theory. This era has proved somewhat destructive to the older logic of science. To describe physical science as a structure of relations

connecting events has been a commonplace ever since the writings of Whitehead, and was even implicit in the Lorentz, Einstein, Minkowski stage of the earliest Relativity: Dirac's more revolutionary approach to atomic physics, and Eddington's pioneer assessing of scientific knowledge as the apprehending of abstract structure, seem to me to make possible a more radical utilization of Russell's analysis of the forms of knowledge. Such analysis might be used in clarifying our minds as to what we really mean by the truth of a proposition or of a theory in physics. The development which I propose seems more hopeful than at the times of writing of any of these prime sources of inspiration, or of Broad, Jeffreys, Ritchie, and other recent pioneers in the logic of science, by reason of Milne's intervening attempt to reconstruct the foundation of scientific argument as a correlation of time-experiences of observers. I have earlier written critically and at some length of this last advance, in a book where Time is discussed as implicit in physical, astronomical, and philosophical inquiry: the contribution of Milne in the present essay is introduced as one item but only one of a variety facilitating a general critique and synthesis of the truthfulness to be claimed in modern physics.

A subject-matter so delicately poised on a borderline between science and philosophy becomes nonsense in any of the common confusions between these territories. To avoid the reproach—sometimes justified—that philosophers pontificate complacently about science while scientists trespass ignorantly into philosophy until the general reader is bewildered and swindled, I have introduced the problem and possible solutions along two main ways of approach. These I have separated as Part I and Part II, although certain notions such as 'structure and form', 'communicability of knowledge', 'coherence as test of truth', are used—with considerable difference—in both parts. I shall attempt to justify in the following General Survey why I have ventured to distinguish in Part I the LOGICAL and in Part II the PHILOSOPHICAL problems of scientific truth. The distinction may also indicate why until the final chapter or Part III, there is no step towards asking what is to be said of the truthfulness of the non-quantitative judgments of aesthetics and morals; in the end these must be brought into brief comparison with that quantitative use of sense-data which we call science, and its interpretation which we must call a philosophy of science.

General Survey

DISTINCTIONS BETWEEN SOME PROBLEMS OF TRUTH FOR SCIENCE AND FOR PHILOSOPHY

1

The property of being called truthful or erroneous characterizes not facts or the actual observations or recordings of the scientist, but his opinions; these are commonly embodied in the scientist's ways of arranging the facts to make theories based upon them. I shall suggest later that theories and hypotheses, and the verbal pictures or mathematical models by which we reduce facts and observations to connected systems of description and calculation, must be assessed as the provisional sorting of a certain kind of statement into a certain kind of pattern: it is to the propositions in this pattern that we aim to ascribe truthfulness or error. A typical proposition would be, for example, an assertion that a certain interaction between electron and electromagnetic field decides the occurrence of some measurable phenomenon in optics or radio, and it is not the fact of the physicist's measurement which can be untrue but the opinion embodied in the explanation.

It is a commonplace of all philosophical and commonsense query about modern physics, that the description of our external world in terms of these 'explanations' has become extremely unlike the description of the same world in terms of the perceptions to which our senses give rise. Nearly every recent writer on the philosophy of science has endeavoured to captivate or to scandalize his readers by pointing out that a lump of material is hard, cold, motionless, impenetrable, in the non-scientific account, but is a whirling swarm of widely separated electric charges in the scientific account; 'matter' reduces to points of singular intensity of electric field in empty space, its constituents totally inaccessible to the sight, touch, smell, hearing, of our individual explorations of the world.

The first question which the physicist must therefore expect to face from the outside critic is: 'By what right do you claim any degree of truthfulness in such a fantastic account of ordinary objects?' In spite of an enormous literature, both of the philosophical and of the popular orders, it is a rare and rash physicist who is prepared to answer this with conviction to-day.

I suggest approaching the question in the first chapter of Part I by a revaluation of the traditional logical sequence; this had usually been considered to lead from isolated facts to hypotheses capable of selection through disciminatory experimental evidence, and finally to the discovery and utilization of Natural Law. In this sequence a causal and mechanically describable scheme had satisfied pre-relativity and pre-quantum physics; but its inadequacy for modern needs raises with sufficient urgency the query of the present day, as to what more ultimately can test a theory and what can constitute the truth of some recent explanations, for example in terms of space-curvature.

But it is clear that such a beginning must inevitably appeal to two very differing interests, and that any answers to the questioning may be satisfying to one only of two kinds of person having different intentions when they set out to judge even 'scientific' truth. The working physicist himself, if he is not a philosopher too, wants a Truth which is measured by the extent to which the science is thereby advanced; to him an error is simply a false start, or any track subsequently to be retraced and abandoned with resulting retardation of progress. By 'progress' he will probably imply the co-ordinating of separate explanations into a connected single group which either brings aesthetic pleasure or facilitates control over natural phenomena. But by whatever compromise between sectional interests in electrical or optical or atomic research, he will certainly confine 'truth' to some such dependence on a provisional plan of importance to the state of the science at that epoch. In effect, his standards will be internal to his science; he will not, as physicist, be concerned to criticize his notion of truth by comparison with other modes of judging the validity of arguing about the external world, and above all he will not concern himself with speculations about any INTERNAL world of the scientist's own mind. Without pandering to the sense of superiority with which many scientists like to imagine that they are 'not metaphysical', he will

reasonably feel that a somewhat pragmatic convention is sufficient for deciding 'what will work' within physics. Part I is accordingly the physicist's reply to his colleagues and students and to his own intellectual conscience: it is not the philosopher's reply, though even the most strictly physical becomes a discussion in LOGIC as soon as the grounds for validity of an argument are to be scrutinized. It is not until Part II that the philosopher (who is certain also to be a logician and may happen also to be a physicist) begins to demand to know what scientific knowledge further means, as defining a particular mode of reacting to the external world, and not merely how it works within the framework of the physicist's own interests. It will not be until the final chapter or Part III that comparison with 'knowledge of truth' other than scientific is sought.

2

In assessing the present status of the logic which may be claimed to govern truthful argument within physics, it will be useful to trace with examples how 'mechanical exploration of cause and effect' characterized the emergence of the modern era from Newtonian times onwards. To begin with, it gave overwhelming advantage to gravitational astronomy over the mere geometrical representation of motions in the sky which had contented the ancients and medievals. It was from such successful instances of a field where effects were predictable that a general law of Causality became plausible, the criticism of certain aspects of which in recent atomic physics is leading to widespread revision of what is meant by scientific knowledge. The success of causal laws throughout dynamics, the molecular but not the atomic discussion of gases, and the dynamical theory of heat, followed by the profitable mechanizing of our notions of electrical conduction and the electromagnetic theory of light, carries physics up to the great triumphs which at the beginning of the century seemed a final crowning of such a very simplified world of matter and radiation. Drastic restriction of the possibilities in mechanical explanation only arose when the connection between matter and radiation was demanded, in the atomic theory of Spectra, optical and X-ray and now nuclear Spectra; the quantum laws which followed this restriction

brought ultimately a precise but narrowed scope to the previously accepted notions of Causality. Quite different criticism of 'cause and effect' sequences in Relativity has been followed by much controversy as to the comparative status of empirical and rational explorations in contemporary science: there seems to be intruding a novel scope for purely rational deductive reasoning, which a preceding generation had regarded as outworn and useless for the 'exploratory' phases of a research.

When we find that to-day 'scientific explanation' need not always include a mechanical model or picture in which cause and effect are traced with Newtonian precision, it becomes necessary to examine the attempts which have been made to rewrite the aims of physics in accord with this loss of confidence in the older grounds for certainty. The most striking originators in atomic theory, Bohr, Heisenberg, and the most revolutionary Dirac, have all been moved to make pronouncements about what physical science is really trying to do, which would have bewildered the confident mechanical physicists of the first decade of this century. Eddington, quoting Bertrand Russell's sense of Form in scientific knowledge, has rewritten much of the basis of scientific inquiry as a search for 'structure' or possibly a creation of 'structure'; this conceivably will replace our ancestors' obsession with seeking 'things'. It seems that if the elements of such formal structure can satisfy what Dirac demands, their genesis may be important to compare with Whitehead's genesis of physical concepts as expounded as early as 1920 in his geometrization of common quantitative experience. A profitable approach to the current meanings of knowledge and truth in physics may thereby be opened in directions hitherto not available. We may in the future have to aim at arranging our experimental facts as a formal pattern in symbols whose mechanical significance we neither know nor care to know; but the labour may be quite as worthy as before, and more successful in approach to a new standard of truthfulness than when we claimed to 'know what an electron really is'.

The utilizing of Russell's most general notions of the formal structure of all logical systems, becomes feasible to a degree not possible even to Eddington or Dirac a dozen years ago, since Milne in 1936–40 began to show how much of physical foundations could be deduced from the rational logical laws essential to the correlat-

ing of different observers' experiences of events and their ordering in time sequences.

I propose to suggest a principle wider than hitherto for covering all that seems lasting in these developments, namely that scientific approach to truth is essentially the rearranging of events from individual time-sequences into transformable patterns whose selection must make them 'communicable'. By communicable I shall imply a generalization of the requirement taught us by Relativity, that the transformations shall become adequate over all varying circumstances of all conceivable observers. This demands in progressive achievement a science independent of all possible individual conditions, the germ of which is again traceable to Russell. The test for truth in a physics so aimed becomes a test of the 'coherence' between the differing statements obtainable after all possible transformations of a scientific proposition. Coherence of the patterns from quantitative experience will be discussed in the final portions of Part I, and it is possible that this importation from a logic which has not recently been fashionable may serve to avoid some of the difficulties of the most recent physics.

For readers whose acquaintance has not been in physics, introduction to the relevant technical material is included in each chapter in Part I.

3

Some care is needed against mixing of categories, in claiming to discuss in Part II a philosophy but predominantly a philosophy of science; Part I had been confined to science and to the latter's logic alone, the introduction of such philosophers as Russell and Whitehead in Part I bringing only their contribution to the notion of the logical forms used in the structure of physics. Some workers deriving from Wittgenstein's development of the Cambridge school would confine profitable philosophy almost entirely to the critique of scientific concepts; others at a different extreme would consider the analysis of science so trivial as to provide only minor general philosophic interest. Between these I would appeal for the following distinctions among the ways in which scientific truth might be discussed.

The treatment in Part I has analysed some of the inadequacy in the traditional logic of the inductive sciences, an inadequacy forced upon them by recent atomic physics, relativity, and cosmology: the remedy suggested was also within logic, as scarcely impinging on any wider philosophical problem than the rearrangement of convenient and legitimate methods within science. This remedy was to replace the older procedure of 'progress from hypothesis to natural law on a basis of mechanism and causal sequence' by a more cautious and adaptable procedure of 'selecting or constructing a pattern of relations between temporal experiences of individuals, that pattern being tested by "coherence" until universally "communicable" and thus true'. In this suggested procedure the commonplaces of inductive logic such as measurement, symbolization, and simplification, all survive, and their embodiment into formal Transformations becomes important; but the notion of hypothesis and law becomes somewhat blurred, the turning from the one into the other becoming less sharp, less decisive, and perhaps less desirable than our ancestors thought in their easier age of primitive science.

But any such revised 'methodology', or analysis of scientific aims and processes, derives original inspiration from the source to which Eddington attributed his recognition that physicists study structures rather than things, namely the English Realist philosophers, of whom perhaps the most important for science are Bertrand Russell and C D. Broad. Their central problem was the problem of Perception: they were interested in the obvious and tantalizing gap between what our senses provide and what our minds construct thereon. Now this gap stretches most widely and disconcertingly of all, when the sense-data of the physicist's measurements are finally attributed to an underlying population analysable only as being the elaborate set of concepts woven into pattern by the atomic theorist or the relativist. So the particular detail stressed in this essay, the aspect of physics as the pattern based on sense-data in the individual sequences of observers' time-experiences, seems to offer unusual hope towards a philosophy of what such scientific knowledge might mean and by what criteria its truth is to be tested. This may be a model problem in theory of knowledge, more likely to be within reach of our generation than the wider questions of the meaning of all knowledge which have been left

inconclusive between the labours of the great philosophers down to the present day.

An unexpected feature of our treatment of this particular and restricted problem of the philosophy of scientific truth, is that while it derives its emphasis on Form and Structure from Russell and the English Realists of this century, it has also, I consider, an intriguing possibility of deriving much from a test of truth which is essentially a test by 'coherence'. This surprisingly suggests the Coherence theory of all knowledge, which belonged to the Idealist philosophers, against whom Russell and the Realists were the indignant reaction. It will be very necessary in Part II to see where the possibilities and limitations of the realist and idealist theories of knowledge might so cancel, in the particular case of scientific 'truth', that in this domain alone the formal elements of the one become compatible ~~with the coherence test of~~ the other.

4

To claim that complete agnosticism about the external world of Nature is only escaped through coherence of physicists' patterns, is not an attempt at a restoration of traditional idealism to replace realism in general philosophy, in which wider domain conceivably neither will ever be conclusive: for in philosophy of physics the coherence which is significant is not the idealist's coherence of mental states.

In fact, by restriction to those kinds of sense-data and that kind of knowledge claimed by physical science alone, Part II avoids any direct intrusion into the philosophies which are designed to cover the totality of experience. In a final chapter, however, segregated as a Part III, there are pointed some of the contrasts with philosophy of aesthetic and moral judgments—together with some possible contacts. These are domains in which Time implies not solely the physical sequence of sense-data but the worlds of memory and imagination contributing to personality and not merely to the external structure of Nature, domains where the quantitative ceases to be the only relevant judgment.

Such considerations may serve to outline what kind of a philosophical problem concerning science is attempted in Part II, as sequel to the mere logical rearrangements within physics attempted

in Part I. We shall be bound to the double question, firstly what theory of knowledge for the case of temporal sequences of sense-data can be suggested BY the topics of Part I, secondly what theory of knowledge adaptable FROM general epistemology will be legitimately applicable to the results obtained in Part I.

PART ONE

METHODS OF APPROACH TO TRUTHFULNESS IN THE LOGIC OF SCIENCE

Chapter 1

CRITIQUE OF SCIENTIFIC METHOD

(I) THE OLDER SEQUENCE FROM HYPOTHESIS TO LAW

A physicist or chemist or biologist of the Victorian or Edwardian eras, when asked upon what grounds he claimed any truthfulness in ascribing observed phenomena to an underlying world of atoms and molecules and electric charges, had little reason to quarrel with the accounts of 'scientific method' by which 'validity of argument' was commended in the final pages of any current text-book on elementary logic. It is a feature of the present discussion to recognize that attitudes to our surroundings implied in such method may alter under the impact of recent physical discovery, and that these alterations may be more radical than any in the formal canons by which the validity was guaranteed. In fact, when modern physics affects the logic of our approach to the external world, it affects more drastically the scientist's mental imagery than his formal rules—his interpretation of what kind of truth he seeks rather than the selection of steps to be taken in the details of pursuit. This will require us some day to cease isolating the logic of science from the psychology of the scientist's own mind, an almost untouched subject.

The traditional logic, or analysis of our right to draw conclusions, had stressed a contrast between the INDUCTIVE reasoning of observational and experimental sciences and the DEDUCTIVE reasoning of mathematics and much social discussion: deduction may typically be represented by the fact that, if a certain general statement be true, then one particular statement is CERTAINLY true if it can be shown to be included under the general, whereas induction typically proceeds from many particular statements observed to be true, towards a generalization which may with varying degrees of reliability be accepted as PROBABLY true.

The canons of validity for deduction had stood with only trivial modification for centuries and can be traced even to Aristotle; whereas inductive methods seemed to develop with the earliest systematic experimenters of the modern era, traditionally expounded by Bacon. They were notably formulated by J. S. Mill in the nineteenth century in a manner useful to his contemporaries but inadequate to present needs of scientific research. Attempts were occasionally made by working scientists to rescue the 'laws' of induction from their brief and sometimes contemptuous treatment at the tail-end of the logic text-books, and these writers succeeded in enforcing recognition that experimental scientists might well collaborate with philosophers when logic is to be redrafted: the most serious contributions were perhaps the treatises of N. R. Campbell (1921), Ritchie (1923), and Harold Jeffreys (1931). Meanwhile philosophers began to query the uncertain status of induction, notably C. D. Broad in 1918, and one systematic treatise on logic (W. E. Johnson, 3 vols., 1921–4) has evolved an interrelatedness of all forms of reasoning, deductive and inductive, or rather 'demonstrative' and 'problematic', under wider and more reliable and understandable principles. These latter bid fair to include the work of the experimental scientific researchers of all kinds, and also of those, notably Bertrand Russell, who had evaluated the rational meaning of mathematical argument. He had not shrunk from justifying the beloved science as 'the study in which we neither know nor care whether what we say is true', in reference to the hypothetical character of the propositions constituting the premisses of any purely mathematical deduction.

The working scientist's attitude is not primarily an interest in the logical forms of his reasoning: nor is he readily drawn to interrupting the progress within a science to raise queries as to Truth. 'Truthfulness' is not a word used very freely within scientific circles, and when introduced it is often uncritically made to mean 'that which is sought and which it is hoped to guarantee by using Scientific Method'. Empirically it can often be adequately judged by the criterion of allowing an ever-widening scope of explanation to an ever greater proportion of hitherto isolated phenomena, thus tending to aesthetic satisfaction of a fundamental human instinct of curiosity or tending to facilitate a profitable control over natural phenomena. When attempts are made to break a vicious circle of

defining truth as the target of scientific method and science itself as a pursuit of truth, the conception of 'explaining facts' seems common ground to many divergent interests: these will include some of the most revolutionary aims of present-day physicists. A similar conception will also serve the workers in many other sciences who ultimately benefit by methods devised for physics, and perhaps *all* the optimistic explorers from Leonardo da Vinci through Newton and Faraday to Rutherford, for whom Nature was more alluring than intimidating and whose labour was not quite inadequately expressed by the commonsense of Mill's 'methods of experimental inquiry'. Explanation—however ambiguous we find that word to-day—would not proceed far from any starting point unless it were based in practice upon a strategy of varying separately the selected factors of an experiment and eliminating those which are irrelevant; and that procedure sums up not only Mill but more modern analyses. But what is altering nowadays is the degree of finality expected at the various stages, and also the kind of mental picture into which 'explanation' is allowed to crystallize: our experimental technique in the physical sciences outgrows itself yearly, far faster than our logical technique. However, it may well be claimed that our logical and philosophical THEORY of scientific knowledge and truth ought not to lag behind as lazily as hitherto, now that the thoughtful scientist's attitude of mind is in a state of flux as to what a 'truthful explanation' means.

In particular, I shall urge in later chapters that the target aimed at in accounts of the physical universe is rapidly changing through criticism of the conceptions of 'mechanism' and 'cause', and that distinctions in traditional logic between hypotheses and natural laws must no longer imply sharp and irreversible transitions from the tentative to the final. If the mathematician has recognized that 'form' interests him to the exclusion of any relevance of 'truth', the physicist's 'truth' may itself become very much more convincing through a radical reorientation towards the logical notion of 'form' which in the work of Russell and others has revolutionized the interpretation of the older logic, if not of its rules. In the present section (I) of Chapter 1 the briefest survey of this older logic of the inductive sciences will suffice to outline what the remainder of the chapter is to assess critically in its historical successes and recent failures, prior to the reconstruction arguments of Chapters 2–5.

The isolated and often uncontrolled observations from which the scientific mind begins to find raw material, must be recorded, and when classified and free from the more obvious irrelevancies they readily suggest a 'hypothesis'. For a stage in science not so primitive as the pre-Newtonian and not so introspective as the post-Einstein, such hypothesis nearly always took a recognized form: it was an imagined reference of the data from the sense-perceived objects to other and unperceivable objects which were regarded as 'causes' but whose behaviour was to be of a similar kind merely on a smaller scale—many millions of times smaller. Typically, and very often successfully, explanation of large-scale facts was in terms of an imagined underworld of atoms and molecules; these were supposed to obey laws of mechanism, and to exhibit cause and effect such as had been found to hold when the larger objects perceptible by sight, hearing, touch, etc., moved visibly and tangibly about the obvious world.

Such hypotheses, for example that collisions between molecules acting like submicroscopic billiard balls could explain the conduction of heat in a gas, were capable in turn of suggesting discriminatory experiments which led either to their rejection or modification or 'verification'. For instance it became necessary to attribute to the balls a non-spherical structure or a degree of elasticity. Verification could be claimed when application of known mechanical and causal laws to the supposed 'model' was able to predict some hitherto unobserved behaviour in the large-scale material object under experiment, and when the prediction worked out for the molecular model agreed with subsequent discovery of that particular behaviour in the larger object accessible to direct sense-perception.

A verified hypothesis was by this means transfigured into certainty, and it was considered that a Law of Nature had been discovered, although it was frequently found that a very different hypothetical model was capable of yielding the same prediction: hence the imagined 'model' of molecular structure was not always unambiguous or unique.

In this scheme of inductive logic there could be many minor variations: some writers inverted the distinctions given to degrees of hypothesis, theory, law, in the tentative explorations towards explaining what we are to suppose must underlie observed pheno-